

# THE NORTH-WESTERN MEDICAL AND SURGICAL JOURNAL.

(NEW SERIES.)

---

VOL. VI.

APRIL, 1857.

NO. 4.

---

## ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Etiology of Milksickness: an Inaugural Thesis for the Degree of M.D. in Rush Medical College, Session of 1856-'7.* By G. W. WILKINSON.

The reading in medical books and journals of the nature and cause of disease, and the remedies proper in their treatment, gives the student but a faint idea of the reality. And, as I am but a student, what I write must necessarily be theoretical, if not fanciful. And the subject I have chosen is the more difficult of writing upon from the fact, that the literature of the profession contains but little upon the subject, and that little is in the form of essays occasionally published in the medical journals of the day. In that isolated form it is not easy to arrive at any satisfactory conclusions. I will certainly not be expected to write anything very definite on a subject, on which Dr. Samuel Thompson represents the whole profession as being 'but inquirers and learners.'

Milksickness, as an endemic disease, is found in circumscribed localities through southern Indiana and Illinois, and it is also said to prevail in many parts of Kentucky and Alabama. The disease occurs almost uniformly in late autumn, and dry weather is especially favorable to its prevalence. We find the disease usually on low, marshy, or bottom-land, or the bluffs

# MEDICAL COLLEGE OF OHIO, CINCINNATI.

## THIRTY-EIGHTH SESSION.

The Thirty-Eighth Regular Course of Lectures in this Institution will commence on the **FIRST MONDAY IN MARCH, 1857**, and continue four months.

### FACULTY.

L. M. LAWSON, M.D.	Professor of Theory and Practice of Medicine.
GEO. C. BLACKMAN, M.D.	Professor of Principles and Practice of Surgery and Clinical Surgery.
THOMAS WOOD, M.D.	Professor of Anatomy.
N. T. MARSHALL, M.D.	Professor of Obstetrics and Diseases of Women and Children.
JAMES GRAHAM, M.D.	Prof. of Materia Medica and Therapeutics.
JOHN H. TATE, M.D.	Professor of Physiology, Hygiene, and Medical Jurisprudence.
JOHN A. WARDER, M.D.	Professor of Chemistry and Toxicology.
E. S. WAYNE,	Lecturer on Practical Pharmacy.
S. G. ARMOR, M.D.	Professor of Pathology and Clinical Medicine.
R. L. REA, M.D.	Demonstrator of Anatomy.
ADOLPHUS STIFFINS,	Janitor.

### CLINICAL LECTURES.

<i>Surgery</i> .....	PROF. BLACKMAN.
<i>Medicine</i> .....	PROF. ARMOR, LAWSON, and GRAHAM.

### FEES.

Full Course, \$64. Matriculation (paid once only), \$5. Dissecting Ticket, \$5. Hospital Ticket, \$5. Graduation Fee, \$25.

The Public Commencement for conferring Degrees, will take place immediately after the close of the session.

The Faculty intend to make this Course largely clinical and demonstrative. With this view the Lectures on Practice of Medicine and Surgery will be delivered at the Commercial Hospital. The Anatomical Department will be fully supplied with material. The College edifice is large and well ventilated, and every comfort and convenience will be extended to the Class.

For further information call at the College on Sixth Street, between Vine and Race, or address

JAMES GRAHAM, M.D. *Dean.*

# THE NORTH-WESTERN MEDICAL AND SURGICAL JOURNAL.

(NEW SERIES.)

---

VOL. VI.

APRIL, 1857.

NO. 4.

---

## ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Etiology of Milksickness: an Inaugural Thesis for the Degree of M.D. in Rush Medical College, Session of 1856-'7.* By G. W. WILKINSON.

The reading in medical books and journals of the nature and cause of disease, and the remedies proper in their treatment, gives the student but a faint idea of the reality. And, as I am but a student, what I write must necessarily be theoretical, if not fanciful. And the subject I have chosen is the more difficult of writing upon from the fact, that the literature of the profession contains but little upon the subject, and that little is in the form of essays occasionally published in the medical journals of the day. In that isolated form it is not easy to arrive at any satisfactory conclusions. I will certainly not be expected to write anything very definite on a subject, on which Dr. Samuel Thompson represents the whole profession as being 'but inquirers and learners.'

Milksickness, as an endemic disease, is found in circumscribed localities through southern Indiana and Illinois, and it is also said to prevail in many parts of Kentucky and Alabama. The disease occurs almost uniformly in late autumn, and dry weather is especially favorable to its prevalence. We find the disease usually on low, marshy, or bottom-land, or the bluffs

near by. Though such is generally the case, yet it is not uniformly so, but, as Dr. Byford remarks, 'their physical characteristics are not peculiar.'

We will now proceed to the consideration of that vexed question, the Etiology of Milksickness. There is great difference of opinion among writers on this subject, and the theories advanced are exceedingly numerous; but I believe there are but four of them deserving of particular notice. Those are, the Vegetable, Malarial, Animal, and Mineral. We will consider them in the order we have written them.

There is but little harmony of opinion among the advocates of the first theory, and they are not at all agreed as to what vegetable shall bear the blame of causing the disease.

Almost every person who has lived in a neighborhood where the disease prevails, and who believes in the vegetable theory, will say that he can point out the offending herb; but two of them can scarcely be found designating the same plant. In order that this theory look plausible, it would be necessary that the suspected vegetable be invariably found in localities where the disease prevails, and only there.

Dr. Drake is said to have advocated the vegetable theory; and if his *Rhus Toxicodendron* should be found where the disease prevails, and absent where it does not, then we would not dare deny that it was the cause. But such is not found to be the case.

A farmer on the high and dry land near Annapolis, Indiana, was in the habit of keeping a portion of his cattle over-night in a small lot, in which was no water and but little vegetation. Those cattle were attacked with sloes or milksickness, while the cattle on other portions of the farm were exempt. He excluded his cattle from the lot, and the disease never occurred on the farm afterward. Under the impression that the disease was of vegetable origin, he marked off the lot into narrow strips and went over the whole of it on his hands and knees, examining every spear of vegetation, but found nothing that did not grow equally abundant on other portions of the farm. In this case there could hardly have been a peculiar vegetable there and escape observation; therefore the disease was not of vegetable origin.



I will briefly add another case. In McLean County (this State), a farmer kept two separate lots of cattle in different enclosures, where was neither water nor vegetation. One lot of cattle had milksickness, while the others escaped. In this case the disease could not possibly have been caused by a vegetable product, for the cattle were all fed from the same corn, and watered from the same well.

This is probably sufficient consideration of the first theory, and we pass to the Malarial.

Were this disease of miasmatic origin, we should expect it to occur according to the following propositions:—

*First*—It would prevail in a direct ratio to the prevalence of the diseases that are by common consent called malarial.

*Second*—Wherever malarious diseases occur to any great extent, we should find milksickness.

We are perfectly familiar with the fact, that when heavy rains fall late in the Spring, causing inundations, which leave ponds and sloughs filled with water to stagnate under the heat of the summer sun, it causes whole districts to be so filled with the miasmatic influence that the inhabitants suffer excessively from intermittent and remittent fevers, and in their most grave form; but no one claims that milksickness occurred to greater extent in such cases. So far as I am informed, the past autumn has been attended with less than the usual malarious diseases, and more milksickness than has been before for many years. So we find that if milksickness prevails in a ratio to the miasmatic diseases, it must be in an inverse ratio.

We will now notice the second proposition:—

In Dr. Wood's *Practice*, we find the following language:—  
'Miasmatic diseases are apt to follow the submerging of meadows in order to increase their fertility, the forming of mill-ponds, and the damming of streams for the purposes of navigation. Neighborhoods, before remarkably exempt from disease, have thus become very unhealthy.' This is a fact too well known to admit of question, but no writer affirms that milksickness is ever so introduced along with the miasmatic diseases into neighborhoods where it had not prevailed before.

In western Illinois, we find the stagnant water, the decaying

vegetation, and all the other requisites for the production of miasmata: and, just as we might expect, we find intermittent and remittent fevers prevailing to as great extent, and in as malignant form as in eastern Illinois; yet I am not informed of a single case of milksickness having been produced west of the Illinois River. Mackinaw Creek empties into the Illinois river a few miles below Pekin; and along the Creek to the very point where it empties into the River we find milksickness; while above and below along the River the disease never appears.

I had occasion the past autumn to go into that neighborhood, and stopped at a house near the Creek for dinner. Our landlady told us that we were in the midst of the milksickness, and that their cattle and those of their neighbors were dying of the sloes; but at the same time she assured us we need not be afraid to eat of the beef on the table, for it had been brought from Spring Lake. Now that Spring Lake is nothing but a slough of several miles in length, running almost parallel with the river, and is so marshy that cattle cannot cross it, though it is at some points but a few rods in width. This slough produces an abundant crop of vegetation, which falls down and decays on the mud, and presents the appearance of being the very hot-bed of miasmata; yet cattle graze about the sides of it and on the low bottom-land between it and the river with perfect impunity, and have never been known to have milksickness. But, at the same time, if we go a few miles up the River, to the mouth of Mackinaw, we find cattle dying by scores. Surely no one would contend that there was more malaria about a clear running stream as Mackinaw, than a stagnant swamp like Spring Lake.

This is all we propose writing on this point, and we pass now to consider some peculiarities which distinguish this from true malarious diseases.

*First, of the Odor*—This is described as something so peculiar as not soon to be forgotten when once observed. Practitioners who have treated this disease, say they are often able to form their diagnosis by the use of but one of their senses, and before entering the house. Dr. Byford has been endeavor-

ing, by forming compounds, to produce an odor similar to that in this disease: and he thinks he has found the very article. Here is what he says of his discovery. 'It resembles almost exactly a strong odor of chloroform mixed with animal secretions. Say a smell of chloroform, and the breath of a patient salivated.' Whether or not this is a just comparison, it is not my purpose to decide. That there is an odor accompanying this disease, which cannot be compared nor mistaken for that of any other, is sufficient for my purpose.

Another characteristic is that one attack powerfully predisposes to subsequent ones which may be induced by over-exercition, even though the person should go to a place distant from where the disease prevails. An instance of this peculiarity I propose giving in a subsequent part of this paper.

Next in order is the Animal Theory, which will engage our attention for a time.

This theory is advocated by a few as the only source, and acknowledged by most writers to be one of the sources; if not the only one, of this disease. It has been observed that dogs, after feeding on the flesh of a beast dead of a peculiar disease, are attacked with a disease in every respect analogous to that of which the beast died; and in both instances the disease is called sloes, trembles, milksickness, &c. From this fact we would very naturally infer that the disease in the beast had poisoned the flesh to such a degree that the dog had received it with the ingesta. But Dr. S. W. Thompson would have us think otherwise: and in his Essay, published in the *N.-W. Journal* for Sept. 1854, he writes thus on this point:—'Those attacked are not always the ones taking the largest amount of diseased flesh, but rather such as stay longest in close proximity to the noxious effluvia arising from the carcass: and as the beast usually dies in or close to the locality where the disease is supposed to originate, it is but fair to admit that the dog was subject to the same cause of disease as the cow. And, again, if there is any peculiar essential poison in the dead cow, we should reasonably expect to see the usual manifestation of the poison in the animal partaking thereof, and these effects should be most plainly exhibited in such as had taken the

largest amount of matter containing it.' He answers his own objection very well in these words:—'It is true that some men (and this fact applies equally to other animals) are more susceptible of a given amount of some agents than others, and this may possibly be the case in the present instance.' We consider this not only possibly the case, but extremely probable, especially so when we consider some other facts in connection. Farmers, living in neighborhoods where milksickness prevails, are well aware, that to prevent their dogs dying of the disease it is only necessary that they be so confined that they get none of the diseased meat. I have seen dogs confined within a few rods of a cow dead of milksickness, and was informed that they had learned from experience that their dogs so confined were in no danger whatever of having the disease. Therefore I think it an established fact that dogs do receive the disease from meat, and seldom, if ever, from other sources. The disease is not only communicated to dogs in this manner, but man may also receive it from diseased meat. To prove this point, I will report a case as I received it from one of the suffering party.

Mr. D. C. Wood and family resided in Greenfield, Indiana, and as milksickness had never appeared in their immediate neighborhood they had no fears of it, and observed none of those precautions which are usually observed where the disease prevails. One day in autumn, a man sold beef in the village, and one or more of each family that ate of it were attacked with milksickness. Some of those attacked died, and others recovered, if a person can properly be said to recover who survives. When those persons were simultaneously attacked with similar symptoms, they were satisfied the beef had been the cause, and so they traced it to its origin, and found it had been brought eighteen miles, and from a neighborhood of milksickness where cattle were dying of the disease.

Mrs. Wood's case presents an instance of that peculiarity of the disease already noticed, and which I promised to refer to again. She now lives in Peoria, Illinois, and is still subject to the disease whenever she greatly fatigues herself by over-exertion, and each attack presents all the symptoms of the original one, not even excepting the peculiar odor: and this,

too, in a place where the disease is acknowledged to never occur as an endemic.

Instances of the above kind are so very common, that the popular opinion that the flesh of animals affected with the disease is capable of producing it in man, is amply sustained by facts. And not only the flesh, but the milk and butter may produce the same effect.

Cases are reported where families had abstained from beef, milk, butter, etc. and still were not exempt from the disease. Such cases do not prove that the disease is never received from animals; but only argues that it may be derived from another source: and that such is the case I am ready to acknowledge. For, when we have traced the disease to the animal, we have not yet found the prime cause, as whatever causes it in the cow, may, under some circumstances, cause it in man.

This brings us to the consideration of the fourth and last of the theories.

We are convinced in our own mind that the prime cause of the disease is a mineral substance contained in the earth, at or near the surface, in certain localities; and being taken into the system of man or beast produces that series of symptoms constituting the disease called milksickness. We are not prepared, as we would wish to be, with an array of facts derived from observation and experiment, to show what that substance is; therefore we will have to depend for the proof of our theory wholly upon deductions drawn from the facts that have come to our knowledge. We think it would not be claiming too much to suppose the substance to exude from the earth, and, under favorable circumstances, accumulate on the surface in such quantity as to be taken into the system of the cow along with the grass, corn, or salt, and produce the disease.

What are the circumstances favorable to the accumulation on the surface? Evidently the very dry weather of late Autumn, when there are no rains to wash it away. When frost has killed the greater part of the vegetation, cattle are obliged to graze near the surface to gain sustenance, and thus are more liable to get the poison with their food: and at this season of the year the disease prevails to the greatest extent, and continues till wet weather begins.

We think this theory will account for the appearance of the disease in all cases where we have known it to occur. On the farm near Annapolis, Indiana, we may readily suppose that the cattle in the lot had taken the poison with their salt or food; and the reason that the cattle did not contract the disease on other portions of the farm, was, that the poison was not at the surface in any other place. In the case in McLean County, Illinois, the substance was probably at the surface in one lot, and not in the other.

We are disposed to think that the earth through the whole southern part of Indiana and Illinois contains this substance, though it is not at the surface in all places. We incline to this opinion from the fact that the disease in some neighborhoods appears to be caused entirely by the water of a spring. The mineral in those springs appears to be in too dilute state to cause the disease until the surface water is dried up.

A student of the present class in the College says, that the farmers in some portions of the southern part of this State notice that when their cattle, for want of water in other places, are obliged to resort to certain springs, usually contract the disease; and if kept from those springs they escape. A case of this kind came under our own observation, where the enclosing of the spring, so that cattle could not get to it, was followed by the disappearance of the disease from the neighborhood.

This is all we had proposed writing, and is probably more than we should have written unless we could write more to the point; but, since writing the above, we have noticed an article in the *Bloomington Pantagraph*, taken from the *Edwardsville Advocate*, and headed 'The Milksick;' and notwithstanding the fact, that newspaper articles have by some means fallen into disrepute, and so far as we know are not taken as good authority on medical subjects; yet we will venture to append an extract, which of course we will not consider you bound to receive as orthodox.

'A few days since, while walking over what is familiarly known as Tan-Yard Hill, a place notorious for the milksick, we observed that the earth was spotted with a white mold, not very thick nor in very large quantities—each spot being at the most

not larger than a dime, and frequently several feet from any other. It appeared to have exuded from the earth, and pressed between the fingers had more the substantial properties of a mineral than a mold. It at once occurred to us that this very article might be the milksick. It may possess saline qualities in composition with arsenic, and hence prove a temptation to cattle.

‘In mentioning the existence of this mold or mineral poison to a farmer, well known in these parts, we were informed by him that he had observed his cattle lick it up when they had access to it, and in no instance without taking the milksickness, and in most cases fatally.

‘That the three-leaved poisoned oak is not the milksick, we think is fairly inferable from the fact that it exists in almost all regions where the name of milksickness is unknown, and that many fields where it does not exist, are absolute milksick graveyards.

‘In fact it is impossible that this terrible scourge should arise from any common well-known vegetable. The experience of thirty, forty, or fifty years, would have surely detected and exterminated any poison above ground. It must come from beneath the surface of the earth.’

We have copied the above article for want of better authority, and it may be taken for just what it is thought to be worth.

From what we have written it will be seen, that we believe milksickness to be a disease *sui generis*, and that it arises from a peculiar poison which man generally receives through the animal creation, but not invariably so. We believe it possible for man and ox to drink of the same water—the ox take the disease and man escape; from the fact that the ox drinks more according to size than man: but man may get enough of the poison to affect him, and thus receive it primarily.

January, 1857.

---

ARTICLE II.—*A very remarkable Case of Valvular Disease of the Heart, resulting in absolute Death of a Portion of the Inferior Extremities.* By F. R. PAYNE, Marshall, Ill.

It will undoubtedly be interesting to the professional readers



of your *Journal* to have a brief account of the above wonderful case. All who saw it were struck with amazement at its unparalleled peculiarities. We will endeavor to give a brief and truthful history of the case without referring particularly to the treatment, from the fact that there was nothing peculiar in it, and I was well satisfied from the commencement that it could only be palliative, and the disease must inevitably terminate in death.

Mrs. Margaret English, aged fifty-five years, living on a farm; she was rather corpulent. Some years ago she was thrown from a horse, and since that time has complained of palpitation of the heart. Last fall she had a violent attack of bilious fever. During that time I visited her daily; her pulse was intermittent during the entire progress of the disease; she frequently had great dyspnoea. In eight or ten days the fever subsided, and she was again enabled to attend to the ordinary duties of the house, but was troubled with what she called 'palpitation.'

*Feb. 2d*—She is now suffering with excessive pain in her legs; they are very cold; there is no discoloration of the skin; pulse not very frequent, but intermittent; the rational and physical symptoms clearly indicated disease of the heart. Tongue partially covered with a brown fur; bowels costive. Ordered blue mass to be followed with powders of quinine and opium. Mustard and stimulating liniments to the cold and painful limbs. After this, veleriated tinct. ammonia three times a day in tea-spoonful doses.

—*4th*—Called in great haste. She has felt better until to-day; but now has a return of pain in her legs, suffering almost intolerable; some heat in the limbs; veins congested, and blood very dark; pulse feeble and intermittent; distressing dyspnoea at times.

—*5th*—Some less pain in her legs; action of the heart the same; there are many red spots on her legs, but no heat, rather an icy coldness; partial loss of sensation in the toes.

—*6th*—The symptoms about the same, except the red spots which are now confluent; toes shrunken, dry, and lifeless.

—*7th*—There is a general red appearance of the skin of

the legs; the toes are dry, hard, and present a reddish transparency—they are absolutely *dead*, dry, stiff, and transparent; and there is but little sensation in the deep red skin below the knee; she has some appetite and feels easy.

*Feb. 8th*—Complains of excessive heat in her limbs, but they are icy cold to the touch; toes as before; intolerable pain in the large muscles of her legs, and feel like they are firmly compressed by an iron band. The skin presents a dark reddish appearance, and is absolutely dead, hard, dry, and contracted, except a small patch on the top of each foot; some red spots above the knee; action of the heart about the same.

—*9th*—She is more comfortable; pulse the same; the red spots above the knee are closer and more diffused, the skin beneath them is perfectly dead, dry, and contracted.

From this time until the 24th, there was but little variation in the symptoms and appearance of the parts. I visited her every day, two or three times. Drs. H. R. Payne, and L. C. Churchill carefully examined the case. There was but one opinion in relation to the nature of the disease. The physical and rational symptoms seem to clearly indicate valvular disease of the heart, and they agreed with me that the treatment could only be palliative.

—*25th*—The skin on the upper part of right foot has sloughed, but this process has not extended beyond the limits of the patch that has retained to some extent its normal color. No sensation in the legs, except in the large muscles; red spots begin to appear on the back of her hands; action of the heart very feeble and intermittent; bowels very torpid.

—*26th*—No more sloughing; the limbs begin to shrink; the dead skin is wrinkled, and seems partially detached from the muscles—when pressed upon, we have a crepitating sound; severe pain in the region of the heart; she rests better when propped up in bed; there has been no mental derangement during her sickness. The death of the skin did not extend after this date, but the affected limbs continued to shrink. It is now utterly impossible to get an action on her bowels by ordinary means.

She died on the second day of March.

This was truly a wonderful case, and we would be pleased to have the opinion of yourself and others in relation to its peculiarities. The case clearly proves that disease of the heart may terminate in death of the limbs—a fact that has never been noticed by any medical writer with whom we are acquainted. I regret that I could not obtain a post mortem.

---

ARTICLE III.—*Injuries of the Knee-Joint.* By BENJAMIN WOODWARD, M.D. of Sharon, Ill.

On the evening of the 4th of April last, I was sent for, in conjunction with Drs. Morey and Pomeroy, of Geneseo, to see George Rose, a German, twenty-one years of age, who had received a severe injury of the right knee, from a large circular saw. We found the whole soft parts of the knee torn into shreds. The outer condyle of the femur was cut through. The patella was also fractured, and nearly one-half of it torn loose from its attachments, and had to be removed. The whole cavity of the joint was filled with saw-dust, and the integuments so much torn that, after removing no more than was absolutely necessary, we could by no means close the wounds. As the patient was young, vigorous, and of good habits, we concluded not to amputate, extensive as the wound was, but to endeavor to save the limb. We, therefore, cleaned and dressed the wound, and secured the limb as for compound fracture. The case was from this time left in my hands. Absolute immobility of the limb was secured, and water-dressings and a low diet depended on.

On the second day violent inflammatory action took place; this was combated by blood-letting, and mercurials and opium. To this treatment the inflammation yielded, and about the sixth day suppuration commenced. After a time this became so excessive that it became necessary to resort to the free use of quinine and the mineral acids. I also had the wound syringed twice a-day with a solution of tannin in water; he was also put on a generous, but unirritating diet.

Varying the treatment from time to time, as circumstances indicated, he so far recovered, that by the 1st of October he was able to walk with the aid of a crutch; and, at the present

time, is able to be at work, but with complete anchylosis of the joint.

Throughout the whole of the treatment, the greatest difficulty was with the condyle of the femur, the suppuration seeming to prevent union of the bone, and union did not take place till the suppuration became much less profuse.

A somewhat similar case occurred to me in February, 1850. A Mr. Rowe, a young man of good health and habits, was wounded by the bursting of the breech of an old musket, which was heavily loaded, and rammed with pounded brick. This was laid on the ground, and R. stooped down to fire it, when it exploded, and the whole charge struck his knee. The upper part of the tibia was injured, and a piece of iron one and a half inches long and half an inch wide, and the thickness of the gun-barrel, was forced under the patella. The whole knee was much injured.

This case I treated much as the first detailed, and the result was recovery, with partial stiffness of the joint.

Injuries of the knee-joint are of so serious a character, that it is generally thought that amputation is the safer course; but I think I am warranted in the belief, that where the general health of the patient is good, and his habits have been regular, and especially if he have the advantage of youth on his side, many such cases may be cured without resort to the knife. I have in many cases found the free application of water, the best means for combating local inflammation, and I have as often found the solution of tannin efficacious in moderating profuse suppuration.

---

ARTICLE IV.—*Clinical Cases treated in the Surgical Wards of the Mercy Hospital, during the Session of Rush Medical College, for 1856-'7.* J. W. FREER, M.D. Attending Surgeon.

Michael Barren, admitted Oct. 20th, with simple fracture of the upper third of femur. Discharged Dec. 15th.

Jas. Gillman, admitted Jan. 6th, with simple fracture of the upper third of femur. Improving, but still in Hospital.

#### FRACTURES OF LEG.

Jno. H. Poor, admitted Dec. 15th, with fracture of the lower

third of tibia. This case, owing to the limb having been badly frozen, is still under treatment.

Luke Harny, admitted Jan. 24th, with simple fracture of leg, of nineteen days standing. The history of this case may perhaps interest the advocates of the starch-bandage in cases of fracture.

The patient received an injury, Jan. 5th, which resulted in a fracture of both bones of the right leg, with considerable contusion of the soft parts. His physician directed the limb to be placed on a pillow, and cold to be applied by means of cloths dipped in cold water. The above treatment was continued for one week; the tissues in the meantime having assumed a healthy appearance, a starch-bandage was applied.

The patient at this time inquired of his physician, 'how long he should remain in bed,' and was informed that it would probably require six weeks for the bones to unite. The patient thinking that the Doctor's object in keeping him in bed so long was merely to make a fee, became highly indignant, and dismissed him, informing him 'that he, [the patient], knew a natural bone-setter who could cure him in two weeks; that he would not submit to being confined to his bed for six weeks; and that he would get up as soon as the bandage dried.' How many days he remained in the recumbent posture after the bandage was applied, we are unable to say, but that he walked a half a mile on the 24th of January we are positive.

At the time of his admission (January 24), the bones seemed to be in perfect apposition and tolerably well united. The bandage being loose, a new one was ordered, and the patient directed to remain quiet; this, however, he obstinately refused to do. After stating to him the danger he incurred, &c. he was permitted to use his own discretion, in regard to walking about. Seeing that we were not disposed to compel him to remain still, he walked about the wards every day, and most of the time without crutches.

On the 9th Feb. the bones were found, on examination, to be so perfectly united, as to no longer require a bandage, and the patient was discharged.

Pat Murphy, admitted Jan. 17th, with compound fracture of the upper third of tibia.

James Welch, admitted Feb. 17th, with fracture of the upper third of tibia, and head of fibula.

The two last cases are still under treatment, and doing as well as the nature of the injuries will admit.

Jas. Carbine, admitted Jan. 24th, with fracture of outer third of clavicle. Discharged Feb. 16th.

#### EXTIRPATION OF TESTICLE.

Andrew Keenan, admitted Nov. 23d, with hypertrophy of right testicle.

The history of this case, so far as we were able to learn, was as follows:—Two years previous to his admission into the Hospital, he contracted gonorrhœa, which subsided in two weeks under the internal use of balsam of copaiba and injections of nitrate of silver. Three weeks after the subsidence of the gonorrhœal discharge, he noticed a slight swelling in the right testicle, with some tenderness. He immediately consulted a physician, who compressed the testicle by means of adhesive straps. Finding, however, at the end of two weeks, that no impression whatever had been made upon the condition of the testicle, notwithstanding the straps had been applied frequent enough to keep up an uniform degree of pressure, he abandoned their use. From that time until within six months of his entrance into the Hospital, he had been subjected to no treatment. Finding that the testicle had already reached such a magnitude as to be inconvenient from that cause alone, he consulted a surgeon with a view of having it removed. This the surgeon declined doing, but directed him to use iodide of potassa, in eight grain doses, three times a-day, and to apply a liniment composed of equal parts of olive oil and tinct. iodine as a local application. The patient followed the above prescriptions for three months, but finding the testicle still increasing in size, abandoned their further use.

At the time of his admission, the testicle, upon examination, was found to be very much indurated, highly sensitive to the touch, and to measure nine inches in its greatest circumference.

The patient being very solicitous to have the testicle removed without delay, the usual operation for extirpation was performed

on the day of his admission. Nothing unusual occurred during or subsequent to the operation. The wound healed kindly, and the patient left the hospital quite well on the 26th of Dec.

The tunica vaginalis was found adherent to the body of the testicle in several places, and, on laying the testicle open, its structure appeared much altered. It differed from the simple sarcocele, there being considerable fatty deposit; and, in one place, about the size of a pea, the texture was reduced to a semi-fluid consistence, somewhat resembling the softening of tuberculous deposits: and yet the whole organ had not the irregular, or knotted appearance of a tuberculous testicle, as described by Dr. Smith.

#### AMPUTATIONS.

There were, also, two cases of amputation of the thigh, one of which was for disease involving the cartilages of the knee-joint; the other, for a malignant tumor involving nearly the whole leg. A full account, however, of the two latter cases will be given in the next number of the *Journal*.

---

## SELECTIONS.

---

### *Editorial Correspondence of Atlanta Medical and Surgical Journal.*

PARIS, December 7th, 1856.

*Dear Doctor*—Exclusive systems of practice in medicine, and methods of operating in surgery; or perhaps I should be better understood if I were to say, hobbies in the practice of medicine and surgery, have done as much to retard the progress of science as any one consideration. All combat the principle, yet all are to some extent culpable; and, worse, it is usually those who have position and influence that are most persevering in forming the ories, and proposing operations, and for a series of years bending facts, however stubborn, to suit and support their peculiar views. No one who is the least familiar with the history of medicine and surgery, will, for a moment, doubt the truth of this proposition. Let us, then, all be upon the alert, watching ourselves as well as our neighbor, and if we should see anything rational attended with happy results, to whatever



extent we may be enraptured with our own peculiar views, let us not pass it by as unworthy of consideration, but stow it away until circumstances shall make it practicable. There are accidents and deformities requiring operations which, in different cases, differ so much in extent, position, &c. that no one method of operating, however successful in the majority of cases, can be applied with the same happy result in all. Vesico-vaginal fistula, which is at present exciting considerable attention, and for which several new methods of operating have been proposed within a few years, is an example of this class of accidents. In this loathsome lesion, as in many others, it is evident that no one method can be alike applicable in all cases; that the operation proposed should be dictated by the position and extent of the lesion, and other considerations surrounding the case, rather than by our predilections for any favorite method; and that the greater the number of successful methods proposed, the greater will be our resources. With this view, I have thought it not amiss to record here the method adopted in a case some time since, by M. Nelaton, which although not new (the figure of eight suture), still the manner of introducing and extracting the pins is certainly worthy of consideration.

The fistula was small, rather transverse, extending more to the right than to the left of the median line, nearer the neck of the bladder than the neck of the uterus. After bringing down the neck of the uterus with Museux's forceps, the os externum being previously dilated, the fistula could be readily seen; the edges were now freshened with considerable loss of substance, removing the entire cicatrix—a procedure which M. Nelaton considers essential to the success of the operation. Three ordinary suture pins, with a thread firmly attached to the head of each, were now introduced as in an ordinary wound, and the freshened edges brought accurately in contact by means of a ligature passed around the pins in the form of a figure eight. The pins were so directed that the head of each pointed towards the mouth of the vagina, so that by means of the threads attached, which passed out of the vagina, they could at any time be extracted without distending the vagina, or otherwise interfering with the wound. A catheter was secured in the urethra. On the fifth day after the operation the first pin was removed by means of the thread attached; two days after the two remaining pins were removed in the same way; the urine all the time passing through the catheter. Two weeks after the operation the catheter was removed, when it was found that she could, for a length of time, retain her urine, passing the whole per urethra—demonstrating the entire success of the

operation. Upon examination some time after, it was found that the wound had united regularly in its whole extent. It is evident that this method cannot be adopted in all cases, but when applicable, is certainly rational, and, from the above result, may be performed with success.

Since my last letter, we have had, in Paris, the 'Blue Man,' Butler, of New York. He, for several days, occupied a bed in the wards of M. Nelaton, and during his stay was the centre of attraction. Butler, as you are apprised, is a striking example of the discoloration of the skin by the long and continued use of nitrate of silver, administered in his case with the hope of relieving an inveterate epilepsy, with however no very favorable result, as he is at present in Europe to be treated, not for the discoloration of the skin, with which I believe he is rather pleased, but for his epilepsy. This case certainly proves the permanency of such discolorations, as it has for eighteen years resisted all attempts to remove the deposit. Butler, it appears, has been submitted to quite a variety of plans of treatment for the relief of his epileptic attacks; more recently, Dr. Green, of New York, cauterized the larynx and trachea by means of the probang, with no favorable result. Dr. Parker, of the same city, proposed castration, the proposition, as Butler says, having for a basis the favorable result of some cases operated on by a physician of Georgia, whose name I cannot at present recall—you, however, I am confident will recollect the report. Butler crossed the Atlantic to place himself under the care of Marshall Hall, who saw him during his recent visit to America, and proposes to cure his epilepsy by tracheotomy. Upon his arrival in London, and finding Dr. Hall absent for some length of time, he visited Paris to have the opinions of some of the most prominent physicians. Not finding M. Nelaton disposed to favor any operation, but to experiment upon the deposit, he left the Hospital and Paris for London, I suppose.

The discussion at the *Academie de Medicine*, upon the treatment of ovarian dropsy, so frequently referred to in my letters, has been closed, not before, however, all the members of that savant body, who were disposed, had expressed their views upon this complex proposition. As remarked in a previous letter, it is not convenient here for me to follow this discussion in all its meanderings, and were I to attempt it, I doubt very much whether I render the subject less complex—whether I should not, by recording the diversity of opinions, add to the confusion which already exists. Much has certainly been said, both for and against the interference of the surgeon, and much upon either side to no purpose. As, in almost all discussions, there

has been two extremes, both, perhaps, alike irrational—the more practical occupying a happy medium. It is evident, however, that the old doctrine of non-intervention, if you will allow the expression, as regards the radical treatment, has received a blow from which it is not likely ever to recover.

M. Velpeau closed the discussion; his remarks, upon that occasion, were entirely practical, and was certainly the best solution of the whole subject that has been given during the discussion. So important do I consider this last effort, that I give it below (in substance) at the risk of worrying you with this subject. That it may be better appreciated, I propose before doing so, to make an extract from a memoir read before the *Académie* by M. Creuveilhier. In this memoir we find the following in regard to the varieties of ovarian tumors:—

‘Cysts of the ovary do not always constitute the same anatomical lesion. The question of treatment which should always be surgical, will depend to a very great extent upon the character of the cyst, as regards the liquid it contains, the disposition of its walls, and its structure. As to the liquid they contain, they may be divided into *serous* cysts, containing limpid serum or serum of various colors; into *albuminous* cysts, in which the liquid resembles the white of an egg; and into *gelatinous* cysts, the contents resembling jelly: these distinctions, although extremely important in a therapeutical point of view, are not always easily established by the character of the fluctuation.

‘As regards the disposition of the cysts themselves, they may be divided into four varieties: firstly, *unilocular* cysts; secondly, *multilocular* cysts; thirdly, *areolar* or *vesicular* cysts; fourthly, *compound* or *complicated* cysts. The latter being the result of the association of an unilocular with a multilocular cyst, or either of these with the areolar or vesicular; again, a cyst may be considered compound or complicated, when it has for its basis, a fibrous tumor of the ovary.’

The above is perhaps the most complete as well as most practical division of ovarian cysts. It is true that several of them are extremely rare, yet all have been recognized.

M. Velpeau’s conclusion was in substance as follows:—

‘The question at present under discussion before the *Académie* is extremely complex. For its solution, it is necessary to determine, *firstly*, the ordinary duration of the disease; *secondly*, its gravity; *thirdly*, the value of the various plans of treatment to combat—that is, to determine, for example, whether a radical cure ever follows the simple puncture, and in what proportion of cases, and if the treatment by injections be proposed, to determine whether iodine is better than all others; and,

*fourthly*, to be able to distinguish the various cysts.

'The first proposition—the duration of ovarian cysts—can it be determined? What has been said here in regard to their duration is far from being exact. Two, six, and ten years have been given as their term of duration; how have they arrived at such conclusions? At the commencement—that is to say, when the cyst has but the size of an egg or an orange—it is seldom recognized; and often, when recognized, and the physician called, it is extremely difficult to determine the date of its origin: again, are not all apprised of the very great difference in the march of serous collections? In dropsy of the tunica vaginalis, for example, there are some cases that acquire a development in six months that others are six years in attaining. In some women, again, ovarian tumors are recognized much earlier than in others; thus it is evident that in a delicate woman, with thin abdominal walls, a cyst would be much easier and earlier detected than in a corpulent woman, with thick abdominal walls. It is, then, extremely difficult to determine the ordinary duration of such cysts. I have seen ovarian tumors the size of a large orange, acquire, in less than a year, the dimensions of the head of an adult; and, after arriving at that point, I have known quite a number of women to live four, ten, fifteen, and eighteen years. Taking all into consideration, I am of the opinion, that, in the majority of cases, women attacked with this form of dropsy may live six, and perhaps even more than eight years after the tumor is of sufficient size to be detected; and since, in a number of cases, life is prolonged, without the intervention of any treatment whatever, to fifteen and eighteen years, it would be extremely imprudent to adopt any plan of treatment that would be attended with great danger. But, as it must, sooner or later, prove fatal, it is certainly rational to interfere.'

Arriving at the third proposition, M. Velpeau adds:—Can cysts of the ovary be cured by the administration of internal remedies? The negative response of MM. Cruveilhier and Trousseau greatly surprised me, as I am certain that I have seen cases cured in this way. The idea has been advanced that ovarian cysts, from their isolation from the organism, were not under the influence of internal remedies: it has been asked, how it was possible, under such circumstances, for absorption to take place. I might ask, also, if the organism, under such circumstances, secreted such large quantities of liquid, why it is that, under the same circumstances, it might not be absorbed? This is a question, however, that cannot be determined except by facts. Hydrocele, although very rarely it is true, sometimes presents examples of spontaneous cures.

M. Velpeau here mentioned a case of hydrocele, in which he proposed an operation, which disappeared spontaneously in less than forty-eight hours.

When the cysts, adds M. Velpeau, are very small, this spontaneous or accidental rupture may result in a radical cure, as has been cited in several cases during the discussion; but often death is the consequence. In a report of seventy-two cases of such ruptures, by Tilt, there were thirty deaths; making it by no means a desirable accident—it is not, however, very frequent. I have only observed it twice, and in both cases death was the result. At the commencement of this discussion, I mentioned some cases in which the simple puncture was followed by death. I had no idea at the time these facts were mentioned, that they would occasion the long and deplorable statistics that have been produced in this discussion.

According to Southom, as cited by M. Trousseau, in twenty-one cases of simple puncture, there were four deaths during the first twenty-four hours, three during the first month, and fourteen during the first year. In thirty-six cases reported by Lee, there were three deaths in the first twenty-four hours, six during the first few days, twelve in one year, five in three years, one at the end of six years, and one at the expiration of fifteen years. In a report by Kiwisch of sixty-four cases of simple puncture, there were nine deaths during the first twenty-four hours, six after a second puncture, fifteen after the third, fourth, fifth, or sixth puncture. It would appear from the above, that the simple puncture is extremely fatal in England and Germany; something which I must say, in all candor, does not appear to me possible. We have never seen any such results in France. I said that I had seen four deaths from the simple puncture. They were cases very grave and complicated—gelatinous and multilocular cysts; but no one of them died in twenty-four hours; all lived for some length of time. Within the past thirty years, I have performed this operation quite a number of times, and, with the exception of the four cases above-mentioned, have never seen death as the result of the puncture—all have lived four, five, six, and ten years, some few as many as fifteen years. This, as you see, resembles in no particular the frightful statistics above alluded to. Why is this? Statistics are frequently on the order of *Æsop's Fables*; in which is found all that may be desirable for good or bad. It is more than probable, that the statistics above referred to are not exact; I shall not, however, attempt here their solution. After a review of all that has been said, I am still firmly of the opinion, that the simple puncture within itself is not fatal, and

is generally inoffensive. Will it cure ovarian dropsy? Some cases. I saw one example several years ago, in connection with Recamier and Nelaton. The simple puncture, like the spontaneous rupture, sometimes results in a cure; but it is necessary to say, that such cases are exceptional. Has it no inconveniences? It certainly has. Where we are forced to resort to it frequently, for the relief of urgent symptoms, we, in the end, exhaust the patient. It should be resorted to rarely, and with decision.

The extirpation of the cyst! There is a singular contradiction between the idea that is, with reason, advanced, of the extreme gravity of the operation, and the statistics of the English and American surgeons. I shall not attempt here to explain this contrast. It is very certain, that I shall never dare perform such an operation; and I should here say, in honor to the surgeons of France, that this operation has never found favor with us.

I come now to the treatment by the injections of iodine. This method is not as new as is generally believed—the injection of cysts was practiced during the past century. How is it, then, that this method is creating at present such an emotion? It is natural we should apply to cysts of the ovary, what has been applied to hydrocele; this is exactly what has been done. As the liquid, that was first employed in the treatment of hydrocele, was extremely irritating, it frequently happened that the inflammation went beyond the limits that were then thought necessary to insure a cure, and resulted in extremely grave accidents. This prevented the generalization of such injections. But, as it was demonstrated to my satisfaction, that, for the cure of such collections, it was not necessary to determine in the walls of the cyst a high grade of inflammation, and that iodine, which is much less irritating than wine, produced sufficient irritation to obtain the result desired without the risk of accidents, so frequent from the injections of alcoholic liquids, I saw at once the advantages that would result from applying the treatment of hydrocele to the various cysts, and thus multiplying its use. This question, besides, is not new; it was discussed here in 1846. I then expressed my views before the *Academie* upon this point; they were supported by MM. Berard and Jobert, and combated by Blandin, Gerdy, and Roux. It was at this epoch that several surgeons commenced studying this subject, and that M. Boinet commenced his experiments, which he has multiplied and studied with such zeal and care that this method has been recognized as his—bearing his name.

In the practice of MM. Boinet, Robert, Monod, Demarquay,



Huguier, Briquet, Nelaton, and in my own, we have a total of one hundred and thirty cases of ovarian dropsy, treated by the injections of iodine. Let us see what we will have from the analysis of one hundred and thirty operations. In this number there were thirty deaths and sixty-four cures. Thirty deaths in one hundred and thirty operations! This is certainly a very heavy per cent. and I would be little disposed to defend the operation did I believe that such a per cent. of deaths would necessarily result. Let us go still further and determine the circumstances, if possible, that produced this result. By what accidents did these thirty women die? When I performed the first few operations of this kind, that which occupied me all the time, and sometimes arrested me, was the danger of injecting such a vast cavity, with the great difficulty in determining the character of the cyst upon which I was operating. It has been said here, that the diagnosis of ovarian tumors is attended with no difficulty. My compliments to any of my confreres who are of this opinion. As to myself, I must say, that I have found the diagnosis, in a number of cases, extremely difficult. When these cysts are small, it is certainly very difficult, if not impossible, to distinguish the true cysts of the ovary from other cysts or collections of liquid in that region—as cysts of Wolfsonian bodies, extra-uterine pregnancy, circumscribed dropsy of the peritoneum, &c. &c. When the cyst is sufficiently large to overcome this confusion, and it being recognized without difficulty, there is something still of great importance to determine—to consider the character of the liquid it contains. All know the difference in point of gravity and curability between a serous and gelatinous cyst. Not only does the liquid differ in different cysts, but I have demonstrated that in the same cyst we may have, at different examinations, a liquid which differs greatly in aspect. Thus, a serous cyst to-day, may later become sanguineous, and vice versa. Besides, I have seen cysts in which the first liquid extracted presented the color of blood, and later, the cyst becoming distended, the liquid was serous. It was the result, most probably, of the injection of iodine—thus transforming its walls. I have often had an opportunity of witnessing this transformation in hydroceles—to see a hydrocele, in the liquid presented; the color of blood, to become later serous. I have, by observing such cases, learned to produce this transformation artificially. I have often cured hydroceles of the above character, by converting them, by means of the first injection, into an ordinary hydrocele.

The operation by the injection of iodine, is it attended with danger? What are the dangers? I must acknowledge, that I,



at one time, dreaded greatly the inflammation of the cyst; but I have since learned that it is not to be feared. The inflammation is never intense; and is always limited to the point the liquid touches. Neither do I fear the puncture, as I have learned from experience, that the simple puncture is not dangerous.

Where is the danger then, and how can we explain the development of fatal accidents? My impression is, that the accidents are the result of the employment of the canula or tube, as practiced by some surgeons. The method of operating, in which the canula or tube is left in the cyst, is certainly objectionable, as it is almost always attended with a suppurative inflammation of the walls of the cyst. In the statistics of this operation, the methods of operating have been confounded; there is, however, a very great difference, for example, between the method by canula or tube, and the subcutaneous process; a difference which satisfactorily explains the difference in results. In the subcutaneous method of M. Guerin, there is rarely ever suppurative inflammation, while in the process with canula, there is, as above suggested, almost always suppurative inflammation, which, in the majority of cases, is kept up until the patient is exhausted, notwithstanding the repeated injections of iodine.

Let us see, now, what were the methods adopted in the thirty cases before mentioned, that proved fatal. In twenty of the cases a canula was left in the cyst, if I am not mistaken; then, we may very well attribute the unfavorable results in the twenty cases to this circumstance. There are only ten cases, then, where death may be attributed to the injections of iodine. Ten deaths in 130 operations is not such a great per cent; it is certainly a very satisfactory result in any new operation, where there is such hesitation in its performance. When, then, it is demonstrated that the treatment of ovarian cysts, by the injection of iodine, is not more fatal than the simple puncture, with many more chances to cure, it must be accepted as a method with many advantages.

In resume, then, I will conclude this subject by saying, that cysts of the ovary, the most frequently mortal in a length of time extremely variable, but which may be approximated at a few years, say six or eight, one but little affected by internal remedies, may rupture spontaneously; but such ruptures, although sometimes followed by a cure, are ordinarily very fatal. That the simple puncture does not offer, within itself, any great danger, but that it is, to some extent, objectionable where it is for a long time and at short intervals repeated—as under such circumstances it exhausts the patient; that the proposition to

extirpate such cysts should be rejected; that the methods by injection, although dangerous where irritating liquids were used, have become inoffensive, and of great advantage since iodine has been adopted; that the injections of iodine are, without doubt, of great utility in *serous* cysts. As to the other forms of cysts, the wisest plan is to let them alone.

W. F. WESTMORELAND.

---

*Dr. Edward Brown-Sequard's Experimental and Clinical Researches, applied to Physiology and Pathology.*

EPILEPSY—(Continued).

SECTION XI. I have been led to believe, by what occurs in animals after an injury to the spinal cord, and by some cases observed in man, that the existence of a particular spot capable of producing fits, when irritated, is not rare in epileptic patients. This spot may or may not be the starting-point of an aura epileptica.

In the interesting thesis of M. Bravais (*Rech. sur les Sympt. et le Traitement de l'Epilep.* Paris, 1827, p. 18), there is a case of a man who had fits when he touched himself, or was touched by other persons, on the region of the temporal bone of the right side.

Fernel, according to Esquirol (*loc. cit.* p. 302), saw epilepsy produced each time pressure was made on the upper part of the head.

Rondelet (*Methode Curative des Maladies*, p. 137,) relates the case of a man who had a fit every time his ears were exposed to cold.

In a young man, in whom there was an aura epileptica starting from the left hypochondrium, a simple pressure on this region was sufficient to cause the fit (Tulpius, quoted by Portal, *loc. cit.* p. 180).

While I was lecturing on this subject in Boston, in November last (1856), Prof. E. H. Clarke told me that he had seen a fit of epilepsy produced by pressure upon one of the mammæ.

I have found that irritation of certain parts of the skin by galvanism caused fits in two epileptics. In one of them, it was the skin of the bend of the elbow, and in the other the skin of a portion of the neck and face. There was no sensation of an aura epileptica in these two cases.

Probably in many cases, without the feeling of an aura epileptica, and even without a feeling of pain arising from any part of the skin, the fits are caused by a peculiar and unfelt

kind of irritation, originating from some part of the skin, or from the sensitive nerve of a muscle. Perhaps it will be possible to detect the existence of such parts of the external tegument, or of such nerves, by various means, of which we will speak hereafter. It is certainly impossible to admit that the sensations which exist when there is an aura epileptica are always the causes of the fits, as we know that sometimes they consist only in a feeling of cold, or a kind of tickling or formication, or a slight pain. Such sensations are certainly unable to produce fits, and therefore there must be some other kind of irritation, not felt, existing together with these sensations, starting from the same point, and producing the fit. Consequently, what is essential in the aura epileptica is not what is felt, but an unknown kind of irritation. This special irritation, we repeat, may exist alone, *i. e.* without any kind of sensation. It is the essence of an aura, without any feeling. A good illustration of this view may be found in some cases recorded by M. Pontier (see above, Sec. IX. Case VII.), J. Frank, and Henricus ab Heer. In the curious case we owe to M. Pontier, there was no pain arising from the feet, and, nevertheless, it is certain that an irritation sprang from them, as we find that the fits were prevented by the application of a ligature round the legs, and afterwards by the section of the saphena nerves. In the case mentioned by J. Frank (*Præceps Medicæ Universæ Precepta*, Vol. I. Sec. III. p. 476), epilepsy had come after a disease of the testicle; the scrotum was much contracted during the fit, and, although there was no feeling of an aura, castration was performed, and the patient cured. It is evident that in this case the fits were due to an unfelt aura arising from the testicle. In the case by Henricus ab Heer (cited by Sennert, *Opera Omnia*, Vol. II. p. 489), a young girl had no feeling of an aura epileptica, but as she rubbed her big toes one against the other during the fit, applications of butter of antimony was made upon them, and the patient was cured. It seems that in this case, also, there was, as cause of the fits, an unfelt irritation arising from the toes. It is well-known that worms in the bowels may cause epileptic fits, although they sometimes do not give pain or any other sensation. The irritation producing the fits is then unfelt, as in the preceding cases.

On one side, therefore, we find that an irritation coming from the skin or a mucous membrane may produce fits without being felt; whereas, on another side, when there is the feeling of an aura epileptica, the variety of the sensations, and their feebleness, often show that it is not they which cause the fit, so that we must admit that even then it is a peculiar, unfelt irri-

tation which produces the attack. In my animals, as I have tried to prove in Sec. IV. it is not the pain caused by pinching the skin of a part of the face and neck which produces the fit, but a peculiar kind of irritation. Perhaps the special irritation which generates a fit gives sometimes a sensation quite special also, and which cannot be described. Many epileptics speak of a strange and inexplicable sensation. M. Delasiauve thinks he has been enabled to judge upon himself how a sympathetic fit is produced. He had a sore throat, with an engorgement of the cervical ganglions. The least pressure on these inflamed glands caused a sudden bewilderment (*éblouissement*). The experiment, repeated twenty times, always gave the same result. M. D. says, that if the pressure had been continued he would have fainted, and that there was quite a special sensation, progressing as quickly as a flash of lightning from the diseased spot to the head (*loco cit.* p. 33-34).

In the cases of epilepsy in which there is an unfelt irritation arising from the skin and producing the fits, is it because the irritation causes immediately a complete loss of consciousness, or because it has not the power of giving sensation, that it is not felt? I cannot answer this question positively. I can only say that it is probable that the two things exist.

If we take notice of these three sets of facts:—*First*, That there are cases of epilepsy in which an irritation arising from the skin, or from the neighboring parts, may cause fits without being felt; *Secondly*, That by pressure or galvanization we may produce in a part the kind of unfelt irritation which causes fits; *Thirdly*, That such a part being found, epilepsy may be cured by either the application of ligatures, the section of a nerve, or cauterizations, &c.:—It becomes evident that it is of the greatest importance to try to find out, in epileptics who have no aura epileptica, if there is not a part of the skin or of a muscle from which arises an unfelt irritation causing the fits. To ascertain the state of things in this respect, various means may be employed. If the fits are frequent, and if they come at regular times, it will be found, by placing tight ligatures around the limbs, whether the attacks are due to an irritation coming from these parts or not. Among other means of detecting the existence or absence of a peripheric irritating cause of the fits, I will point out particularly the following:—Pressure upon the various parts of the body; the application of localized and powerful galvanic currents; the application of ice, and of a wet and warm sponge, &c. If any part is the seat of a pain, even if this pain seems to have no relation with the fits, it will be necessary to ascertain whether pressure, galvanism, &c. applied

upon this part, produce an attack. If it is in a limb that a pain exists, a ligature will decide the relation of the painful spot with the fits. In cases where there is a cramp in some of the muscles, or in one only, at the beginning of the fit, the inducement of a cramp by galvanism might decide if the attack is due to the irritation of the sensitive nerve of the contracted muscle, or if the cramp is nothing but a manifestation of the attack. If the initial cramp exists in a limb, an elongation of the contracted muscle, or a ligature, might lead to the solution of the question.\*

The danger of producing a fit by the employment of some of the means that I have indicated as good to decide if there is an unfelt irritation arising from the skin, or from some muscle, and causing the fits, is not a reason to prevent our making use of these means; because the existence of a fit, particularly when we are prepared for it, is a small evil in comparison with the great benefit that may be derived from such a trial.

In my animals, nothing in the skin of the face and neck (except a slight congestion, which perhaps is the result of the pinching, and other modes of excitation that I employ,) indicates that this part has such a power as that which it alone possesses to cause fits when irritated. It results from this fact, that it would be quite wrong to decide, *a priori*, that an epileptic man, in whom the skin seems to be perfectly healthy, cannot have fits produced by an irritation of some parts of his skin. Even in such a case, therefore, it would be necessary to employ the various means I have indicated to decide the influence of the skin on the production of the fits.—*Boston Medical and Surgical Journal*.

---

\* No one will imitate a surgeon (cited by Portal, *loc. cit.* p. 135), who performed an amputation of one of the toes, because the movements of this toe were very violent during the fit!

---

## BOOK NOTICES.

*Transactions of the American Medical Association, Vol. IX.*  
1856. Philadelphia: Printed for the Association by T. R.  
& P. G. Collins.

(Continued from March No.)

The next paper in the Volume of Transactions, is a 'Report

on the Sanitary Police of Cities,' by James M. Newman, M.D. of Buffalo, N.Y.

This is a well-written and interesting paper, occupying fifty pages. It contains statistical tables, showing the ratio of annual mortality in Boston, New York, Philadelphia, Baltimore, Charleston, New Orleans, and Chicago. A comparison is also made between the mortality of cities and rural districts, showing the great preponderance of deaths from certain diseases in the former. In addition to the statistical part of the Report, the writer discusses with ability the subjects of good water, pure air, sewerage, paving, &c. and closes with an appendix, showing the condition and extent of *Tenement Houses* in New York and Brooklyn.

We come next to a 'Report on the Treatment of Cholera Infantum,' by A. J. Fuller, M.D. of Bath, Maine.

This Report occupies only six or seven pages of the ponderous volume before us. But, short as it is, it might have been made much shorter, for the whole of Dr. Fuller's pathology and indications for treatment are contained in the following brief sentences:—

'From the fact of the occurrence of the disease at the time of teething, when the muciperous glands are in a state of great functional excitability, it appears evident that cholera infantum is a disease seated in the mucous follicles of the intestines; and that the hepatic congestion *is necessarily connected with it; the disease of the follicles being a secondary affection, caused by the congested state of the Liver*, by which a free passage of blood is prevented.' \* \* \* \* 'The treatment should be mainly directed to the glandular congestion, on which the disease depends; for, if once a *free secretion of bile is obtained*, the disease may generally be considered as overcome.'

As might be expected from this pathology, the author relies in the early stage mostly on leeches to the right hypochondrium, and small doses of calomel; and, in the advanced stage, on the warm bath, stimulating frictions, mild astringents, and tonics. It is scarcely necessary to state that we regard both the pathology and the treatment as exceedingly defective.

The next paper is a 'Report on the Use and Effects of



Applications of Nitrate of Silver to the Throat, either in Local or General Disease,' by Horace Green, M.D. &c. of New York.

This paper occupies thirty-five pages, and presents an interesting summary of the author's well-known views in relation to the application of nitrate of silver to the larynx, trachea, and bronchial tubes. Although the views of Dr. Green have already been made known to the profession so extensively that a detailed analysis of this report is unnecessary, yet we quote below what he says in relation to direct injections into the bronchial tubes:—

'It was our intention, however, to have illustrated the great value of this therapeutic agent, in the treatment of the different forms of disease, to which we have referred by the history of cases which have fallen under our own observation, which would have corroborated fully the favorable reports made by the preceding authors: but this paper is already sufficiently extended. Justice to this subject, however, would not be done, should we fail to allude altogether to the success which, during the last eighteen months, has attended the still farther extension of topical medication, in the treatment of thoracic disease, effected by means of the operation of *catheterism* of the air-passages, or the injections of a solution of the nitrate of silver into the bronchial divisions.

'During the last eighteen months, or since October, 1854, over one hundred patients, embracing cases of both pulmonary and bronchial disease, have been treated by this form of topical medication, conjoined with appropriate general remedies. The history of this plan of treatment and the results of the practice, results which have been in a high degree satisfactory, have been brought before the profession in papers read before the New York Academy of Medicine; before the State Medical Society of New York; and, more recently, a detailed report, embracing a statistical table of one hundred and six cases, thus treated, was published in the pages of the *American Medical Monthly*. Besides their publication in this country, most of these papers have been reprinted in some of the medical publications of Great Britain, and have also been translated and republished in a few of the leading journals of France. It will, therefore, be unnecessary to bring the whole subject before the Association; and we shall close the present report by a brief analysis of the cases embraced in the statistical table, which, with the history of many of these cases, may be found in the *American Medical Monthly* for March, 1856.



‘Of one hundred cases of thoracic disease treated by catheterism of the air-passages, seventy-one of the sum total are recorded as cases of *tuberculosis*. Of this number, *thirty-two* were considered cases of *advanced phthisis*—cases in which tubercular cavities were recognized in one or both lungs; and *thirty-nine* cases of *early phthisis*. Of the first division—advanced phthisis—fourteen have since died. *Twenty-five* were more or less improved; their lives being apparently prolonged by this method of medication. *Seven* only of the *thirty-two* cases of advanced phthisis were not benefited by the injections.

‘Of the *thirty-nine* cases of incipient tuberculosis, twelve of this disease have apparently recovered. Five more of this number are now, or were, at the time of making the report, in the enjoyment of a good degree of health. With respect to the above twelve cases, I say *apparently* cured; for, although the appearance of these patients, as manifested both by the physical and rational signs, is indicative of an ordinary degree of health, yet, in a disease like that of tuberculosis, every medical man is aware that one year is a period too brief to speak decidedly with regard to the positive and final result.

‘Of the remaining *twenty-two* cases, many of whom, at the time of the report, were still under treatment, *seventeen* had been greatly improved by topical medication; three more had been moderately benefited; while *three* only had failed to obtain any advantage from the local measures which had been adopted.

‘Of the *twenty-eight* cases of *bronchitis*, sixteen had been dismissed cured, or so much improved as to require no further treatment. All the others had been greatly benefited, although some were still under treatment at the time of making the report.

‘Finally, in view of all that has been accomplished by topical medication, the Chairman of the Committee would reiterate the declaration made in the first paper communicated to the professional public on this subject, that “the results of this method of treating disease, whether it has been employed in bronchial affections, or in the commencement of tuberculosis, have already afforded the most gratifying indications that practical medicine will be greatly advanced by this discovery.”\*’

The next twenty pages in the Transactions are occupied by a ‘Report on the Best Mode of Rendering the Patronage of

---

\* American Medical Monthly, Jan. 1855, p. 25.

the National Government Tributary to the Honor and Improvement of the Profession,' by Joshua B. Flint, M.D. of Ky.; and then comes the Report of the Standing Committee on Medical Education, by its Chairman, Wm. Henry Anderson, M.D of Alabama.

From the last-named paper we copy the following, and commend it to the careful consideration of such of our neighbors as have been trying for the last three or four years to make students believe, that clinical instruction in hospitals was of no value in connection with our medical colleges.

'The science of Medicine is such by its very nature, that it can be properly taught only in populous towns. The practical part of it ought to go on *pari passu* with the theoretical. If it do not, the graduate must go forth into the world incompetent to do his duty. If the population surrounding him could judge of his qualifications, all danger would be averted, and the evil would remedy itself. But inasmuch as the masses *must* be ignorant on those very subjects which most conduce to their welfare and happiness, they ought, if possible, to be protected by proper legislation. All educated physicians are aware that practical medicine can only be studied in hospitals and infirmaries; but the desire for popular applause, and thirst for reputation or a name, induce many to seek charters for colleges to be located in any little town or village in which they may happen to reside. The fault exists not in the fact that there are too many medical institutions, but that one-half of them have not the facilities to teach the science as it should be taught. The wants of our population require three thousand graduates yearly, and we believe that this number would be much better prepared at a dozen colleges, than at two or three; but those colleges should exist at such places as could afford sufficient clinical instruction, and, as it would be impossible to draw the line of demarcation which would constitute a city sufficiently populous for a medical college, there ought, in justice to the community, to be a higher tribunal before which all graduates should appear, and be impartially examined before receiving license to practice. If the licentiates of colleges from town and villages would in every case seek populous cities to perfect their education in hospitals and infirmaries, all would be well enough—but the *few* only do this, while the *many* go out into the world unprepared to do anything but injure the community, and degrade the profession by their inability and ignorance.'

The leading distinctive idea in the report of Dr. Anderson,

however, consists in the recommendation that students of medicine become more thoroughly versed in the *elements* of each branch of medical science before attending any medical college.

He says:—

‘It is believed that three-fourths of the medical students who fill our numerous colleges, receive their earliest instruction in the rudiments of the profession in the offices of country physicians, and in small villages, where they get a very inadequate idea of any branch of medicine. If this large number of students could enter college well prepared, they might receive from two courses of lectures a very respectable share of medical knowledge. All will agree that the subjects embraced in a course of lectures include all that is necessary, and that the various chairs as a general rule are filled with competent professors: but the difficulty is that the students cannot comprehend the largest portion of the lecture, and even if they could, a vast amount of time is wasted on subjects that might be learned before entering college. If the student were well prepared, much of the professor’s time might be expended on subjects of a more practical nature. Aware of this fact, a previous report to this Association recommended the establishment of preparatory schools. Much good was expected from the adoption of this plan; but experience has proved that as a general rule it could not be adopted, as it could only be put into operation in towns and cities, thereby effectually cutting off the great number of students from these useful institutions. The idea of a preparatory education was an excellent one, since it is from this source alone that we are to expect any radical change in our present system. How then is the candidate for admission into a medical college to become prepared to enter with advantage his collegiate course? This is a subject to which your reporter has directed his attention especially, and on which he has reflected a good deal.

‘A great amount of matter which, under the present system, necessarily enters into a professor’s lecture, might be learned in the office of the physician, or even in the closet of the student, before he commences a course of lectures. If, then, he can get an accurate knowledge of this large amount of medical matter, he could devote his collegiate period to a much more extended course of study than he could under other circumstances. The professor would not have to waste time on subjects purely elementary, but could plunge at once into important practical matters, which he is now debarred the satisfaction of doing, because he knows he cannot be thoroughly understood.’

To carry into effect his leading idea, the reporter recommends the following project:—

‘The question now arises, From what source is information on all these subjects to be derived? It is true they are all discussed in a clear and practical manner in books already published, but in such books they are so interspersed with other matters as to be of little value to the student who is just commencing his studies. Another book is needed; a book which will contain in one, or at most two volumes, dissertations on the various subjects which enter into the course of medicine. It would be no difficult task for an experienced member of the profession to prepare such a text-book, and there is little doubt that if it met the end proposed, it would have a circulation co-extensive with the *United States Dispensatory* itself. Such a book would find its way into every physician’s office, and the four or five thousand students who are annually commencing medicine, would purchase it as the most valuable preparatory work they could possibly procure. It would enable them to learn everything necessary for a thorough comprehension of the lectures delivered in any college in the United States, and would so lighten their collegiate labors that they might devote a large portion of time to those practical matters which, under the present system, must be neglected both for want of time on the part of the professor, and for want of capacity on the part of the student.’

The evil which the reporter here points out, and the object he wishes to have accomplished, are doubtless real and important; but we fear that he neither fully comprehends the one, nor sees clearly the only practicable method of accomplishing the other. That a large portion of students are not prepared to profit by some of the courses of lectures to which they listen in our colleges, is true. This arises, however, not from the want of a student’s *vade mecum*, or book of elementary facts and principles in relation to all the branches of medicine, but simply because he is obliged to listen to all the branches at once, or during the same day; while, in truth, a thorough knowledge of some of them constitutes the only adequate preparation for entering profitably upon the study of the rest. For instance, the preparation which the student needs to enable him to listen profitably to courses of lectures on practical medicine, surgery, and midwifery, is not a mere smattering or

superficial study of the outlines of these branches, but a thorough and detailed knowledge of anatomy, physiology, chemistry, and materia medica. The true remedy, then, is not in the study of any proposed *book*, or in making the courses of lectures in the colleges less elementary, but in so extending and dividing the annual college terms that each student can attend such a series of lectures as is suited to his particular period of advancement in his studies, without being obliged to include others for which he is wholly unprepared.

---

RIGBY *on the Constitutional Treatment of Female Diseases.*

This is a small-sized monograph, treating of those diseases peculiar to women. The subject is one of the highest importance, and the author's previous *System of Midwifery* a sufficient guarantee to the profession of his ability to render it interesting and instructive.

The first, second, and third chapters are devoted to the consideration of amenorrhœa, dysmenorrhœa, and menorrhagia. He enters as fully into the causes and treatment of these affections as the limits of such a work will admit. In the preface he says:—‘I have devoted separate chapters to the consideration of these subjects [amenorrhœa, dysmenorrhœa, &c.] solely in deference to a long-established custom, which in former times was a necessity, when their nature and essential causes were imperfectly or erroneously understood, and which has been sanctioned by force of habit; but I feel assured that my readers will agree with me in the conviction that the time will come when these terms, as well as that of leucorrhœa will no longer designate distinct affections, but will be classed with such symptoms as pain, rigors, expectoration, &c.’ The next chapter includes the consideration of ‘Uterine and Vaginal Discharges,’ which he divides into two classes:—*First*, Those connected with functional derangement; *Second*, Those connected with organic disease. The five following chapters he devotes to the study of inflammation, ulceration, and displacement of the uterus. His views on these subjects correspond with those of Sir C. M. Clark and Tyler Smith. The subse-

quent chapters comprise the consideration of simple and malignant tumors of the uterus, and of the principal affections of the accessory organs of generation. Each of these subjects are treated with a brevity and interest such as to recommend the work to those whose limited opportunities will not admit of the perusal of a more complete treatise. The work is designed to be a practical one, and of utility to the practicing physician as a book of reference: as such, it claims due patronage.

G. K. A.

---

## EDITORIAL.

### *Death of Dr. Kane.*

We are sure our readers will all be interested in the following account of the sickness and death of Dr. E. K. Kane, which we copy from the *Boston Medical and Surgical Journal*:—

‘The death of the late Dr. E. K. Kane, which took place in Havana on the 16th of February, though not unexpected, has still filled the minds of all who knew him with deep regret that a career so brilliantly commenced and so faithfully followed, should be so prematurely terminated. It was the fortune of the writer to attend him, in consultation with his regular physician (Dr. La Riverend), during the last part of the sickness which terminated his life. A few particulars of his case, gathered in that short period, will, it is believed, derive some interest from their connection with one so justly celebrated.

‘Dr. Kane inherited a decided predisposition to rheumatic affection, and had from early life been subject to attacks of articular rheumatism. He suffered very severely from this disease after his return from the first Arctic expedition. The heart, also, had become involved, and he was thought to have a considerable degree of hypertrophy, together with thickening of the valves. So severely was he afflicted with articular rheumatism while preparing for the last cruise in search of Sir John Franklin, that it was often necessary to apply frictions to the joints for an hour, before rising in the morning, in order to enable him to ride to the Navy Yard, where the “Advance” was fitting out.

‘Very soon after getting into the high latitudes, however, these difficulties subsided—a result which would hardly have



been anticipated, but which he had observed in his own case on his previous voyage. What his sufferings and exposures were during his Arctic expedition, is well known; but it is proper to state that they were much more severe, and their effect upon his constitution more disastrous than would be supposed, from the few allusions made to his own case in the published account of the expedition.

‘On his return, his previous rheumatic and cardiac troubles had become complicated with scurvy; though very much exhausted and worn out by the hardships he had undergone, he allowed himself no time for repose, but labored incessantly in preparing the account of his expedition for publication. This fatigue, together with the great change in climate and habits, brought on a severe relapse of his constitutional disease, aggravated by the newly acquired scorbutic taint. He received little or no benefit from the treatment of his disease while in this country, and was advised to try a change of climate; accordingly after the publication of his book, he sailed for England. Here his health became much better, all his symptoms were much improved, and he considered himself nearly restored to health. As, however, there still remained some traces of scurvy about him, his physicians advised him to spend the winter in the West Indies, for the benefit of the climate and fruits.

‘Since his return from the north, there was a somewhat remarkable change in his ability to bear the motion of the ship; he had become unusually sensitive to sea-sickness, which was brought on by even a slight roll of the vessel. The voyage from London to St. Thomas was, however, well supported; while there his health continued to improve, and at the end of six weeks he sailed for Havana. The ship in which he took passage was overtaken by a severe storm; he was very much affected by the motion of the vessel, and in the effort and strain of vomiting ruptured a blood-vessel in the brain. Entire insensibility followed, and continued for several days after his arrival in Havana. A partial recovery took place after a few days, but the right side was found to be completely paralyzed.

‘During the months of December and January and until the 10th of February, he slowly rallied from this attack, and was able to walk a little about his room and to drive out. He recovered the use of the right hand and wrist to a great degree, and shortly before the second attack was able to rotate the forearm. His mind was perfectly clear, although there was some loss of control over the memory. When he endeavored to recal any circumstance which had transpired, several others, more or less connected with it, were remembered, from which he was



unable to isolate the particular fact desired. Of this difficulty he was himself perfectly conscious.

'On the 10th of February, at the morning visit, he appeared more cheerful than usual, and conversed a good deal with those about him. About 11 o'clock, however, he was suddenly seized with a severe attack of apoplexy, which deprived him entirely of consciousness. There was at first considerable spasmodic action of the muscles, which simulated in some degree a fit of epilepsy. These symptoms soon subsided, leaving him with almost complete paralysis of the entire body. The iris responded to light, and the muscles of the pharynx acted when stimulated by fluid introduced into the mouth. The pulse was feeble, and varied from one hundred and twenty to one hundred and forty beats. The skin was moist and cool. He remained very much in this state until his death, which took place on the fifth day after the seizure. In this interval, however, he seemed to have recovered some degree of consciousness, and several times signified assent to a question by turning his eyes toward the speaker. There was some motion of the lips when a spoon was placed in the mouth, and once or twice he was able to make sensible pressure with the right hand. There was no indication of suffering during his last hours, and he died apparently from simple exhaustion.

'The tenacity of life in this case was quite remarkable. A constitution broken by chronic disease of many years' standing—a series of hardships and exposures almost unheard of, with all the depressing addition of care and responsibility—followed by an affection which for some months threatened his life; add to all these an attack of apoplexy, paralyzing entirely the right side, and in two months after a relapse affecting the whole body, and one can hardly conceive how life could have been sustained for so long a period as five days after the last shock.

'The treatment in this case was quite simple. On account of his previous illness and the scorbutic taint in his system, it was thought unsafe to resort to the active measures usually pursued in such cases. After the first attack, small doses of ext. nux vomica with quinine were administered. These were suspended after a time, through fear of increasing the cardiac disease, and a high tonic and anti-scorbutic course was followed. After the second attack, a few leeches were applied, together with cold applications to the head.

F. S. AINSWORTH.'

---

*Delegates to the American Medical Association.*

The following are the names of the Delegates appointed by

the Illinois State Medical Society to attend the Annual Meeting of the American Medical Association, to be held on the *First Tuesday* in May, in the City of Nashville, Tenn. viz:— Drs. A. H. Luce, of Bloomington; Sam'l Thompson, of Albion; J. V. Z. Blaney, of Chicago; T. R. Edmiston, of Clinton; J. M. Steel, of Grand View; S. T. Trowbridge, of Decatur; S. W. Noble, of Leroy; J. M. Williamson, of Lovington; F. B. Haller, of Vandalia; W. A. Hillis, of Hillsboro; J. N. Cameron, of Charleston; and Thos. Hall, of Toulon.

---

*Annual Meeting of the Illinois State Medical Society.*

The next annual meeting of this Society will be held in the city of Chicago, commencing on the first Tuesday in June next. We trust that all the local societies in the State will bear this in mind, and appoint such delegates as will attend. We would remind members of the profession, residing in counties where no local societies exist, that if they attend, with evidence of their being graduates and in good standing, they will be cordially elected as permanent members and be allowed to participate in the business of the meeting. We confidently anticipate a full and profitable meeting of the Society. The following is the Committee of Arrangements, viz:—Drs. J. H. Hollister, De Laskie Miller, J. Bloodgood, C. G. Smith, and Henry Parker.

Dr. J. V. Z. Blaney, is also Chairman of the Committee on Prize Essays; and all papers intended for that purpose, should be placed in his hands on or before the first day of May, 1857.

---

*Agent.*

Mr. John B. White, of Aurora, has been duly authorized to act as Agent for this *Journal*, both for soliciting new subscribers and collecting money from those who are already in arrears. But many of those who are indebted to us for one or more years' subscription, live off from the railroad lines, and so scattered that it is very inconvenient for an agent to visit them: we hope all such will speedily remit to us by mail, without waiting for an agent to make personal application to them.

*Medical Controversy.*

The two medical journals published at Detroit, Michigan (the *Peninsular* and the *Independent*), have been at war with each other until they have begun to publish certificates, or letters of sympathy and approval from their respective friends. This is certainly no very flattering indication on the part of either, and we would with all kindness advise them to heed the dictation of the *American Medical Gazette*, whose editor, Dr. D. M. Reese, has authoritatively commanded them to hush their belligerent passions and in future to keep the peace.

---

*The Influence of Alcohol on Tuberculosis.*

Our Address on this subject, read to the Illinois State Medical Society at its last annual meeting, has been much more extensively copied and favorably noticed by the medical press of the country than we had any reason to anticipate. But now and then an editorial *confrere* takes us all aback, and disposes alike of our facts, figures, and theories with a *dictum* worthy of *Sir Oracle*. As a specimen we copy the following from the March number of the *Charleston Medical Journal and Review*:

“Dr. N. S. Davis, the President of the Illinois Medical Society, in his Address on the “Influence of Alcohol on Tuberculosis,” takes occasion again to express his very erroneous notions, on the *modus operandi* of the alcoholic stimulants.

“In answering the inquiry, what are the effects of alcohol on the human system? he has come to the following conclusions:—

“1st. The presence of alcohol in the blood produces a temporary exhilaration or excitement of the nerve structures.

“2d. It diminishes the excretion of carbonic acid from the lungs; diminishes the change of color from venous to arterial blood, and diminishes generally the organic changes in the system.

“3d. It depresses the temperature of the body, and lessens the tone of the muscular structures.

“Some of the audience may be ready to ask, if I would deny the almost universal sentiment of mankind, by claiming the broad ground, that *alcoholic liquors are never tonic, invigorating or supporting to the human system?* I answer *unhesitatingly, Yes!* and base my answer, not merely on the deduc-

tions of the most careful and varied experimental researches, but ask, in return, what there is in the tottering and unsteady step, the impaired digestion, the frequent functional derangements of the kidneys, exhibited, in a greater or less degree, by all habitual drinkers, which is indicative of increased physical strength, vigor, or power of endurance?"

'Here we find, not only the deliberate expression of an opinion, in direct opposition to the experience of the most learned and acute observers, but a repetition of the stereotyped error, of confounding under one name the various and diverse products of fermentation and distillation, and also of confounding the abuse with the use of these diverse agents. Dr. N. S. Davis, as Professor of the Practice of Medicine in the Rush Medical College, and Editor of the *North-Western Medical and Surgical Journal*, with six lines of titles appended to his name, must, be an influential member of the profession in the North-West, and his opinions must there obtain some weight; but we do not, even for all this, deem it necessary to undertake a refutation of the views above expressed, but leave them to suggest their own reply.'

We cannot refrain from expressing the wish that the erudite editor of the *Charleston Journal* had descended from the task of counting the 'lines of titles appended to his [our] name,' and informed us specifically which of the three propositions quoted by him are included in the catalogue of '*very erroneous notions*.' We presume it is not the first, for we are not aware that any one denies the exhilarating or stimulating influence of alcohol on the nervous system.

Is it the second? If so, he must deny the direct analytical experiments of Drs. Prout, Bouchardet, Magnus, Bocker, and some dozen other experimenters of acknowledged authority in Europe and America. But, perhaps, in his estimation, the results of direct experimental researches are not to be regarded when they chance to stand in 'opposition to the experience of the most learned and acute observers.' Can our Charleston friend tell us how far the 'experience of the most learned and acute observers' in Harvey's day corresponded with the results of his experiments in relation to the circulation of the blood? Or will he tell us how long it is since the '*experience* of the

most learned' in the profession was all given in favor of keeping consumptive or tubercular patients carefully protected from cold air and exercise, and restricting them to sedatives, counter-irritants, and low diet? Does the history of medicine show, that what is called 'experience,' even of the 'most learned and acute,' can be safely regarded as infallible?

But it is probable that our 'very erroneous notions' are contained in the practical inferences drawn from the three fundamental propositions stated, rather than in the propositions themselves. Our chief inference was, that an agent which, while it excites the nervous system, also directly lessens the interchange of carbonic acid gas and oxygen in the lungs, diminishes the organic changes in and eliminations from the body, and indirectly diminishes the temperature, could not be either tonic or invigorating to the human system under any ordinary circumstances. Believing tuberculosis to result from a defective condition of the assimilative and nutritive functions, we did not see clearly how alcohol could act either as a preventive or curative agent in the treatment of that disease. Wishing, however, to test all inferences by actual facts, we canvassed the domain of medical literature on the one hand, and kept a careful record of what transpired under our own observation on the other. From the first we found many general facts of much interest, some of which we would commend to the careful consideration of our friend of the *Charleston Journal*:—

*First*, The general statistics of mortality show, that those communities and nations in which the greatest quantities of alcoholic beverages are consumed, present the highest ratio of mortality from pulmonary and tubercular diseases.

*Second*, In all the large cities where the consumption of alcoholic liquors is well known to be much greater in proportion to the population than in the country districts, the ratio of mortality from tubercular diseases is also much in excess.

*Third*, The army statistics of both England and America show, that in the armies of both countries, made up mostly of young men taken originally from the more hardy and healthy classes of society, and nine-tenths of whom, after their enlistment, have their regular rations of alcoholic drink daily, the

ratio of deaths from tubercular consumption largely exceeds that of the ordinary population of the countries to which they belong.\*

*Fourth*, Wherever statistical comparisons have been made concerning the absolute sickness and mortality among bodies of men of the same class, and living under the same circumstances, except that one part of them use alcoholic drinks and the other do not, the result has been uniformly in favor of the latter.†

*Finally*, We turn to a careful record of such individual cases as have passed under our own observation, and find here too no evidence of either the prophylactic or curative effects of alcoholic liquors in tuberculosis; but, on the contrary, a higher ratio of its prevalence among those who use such liquors than among those who do not.

And what does our Charleston critic offer us as an offset to all these facts, general and special? Does he adduce facts of an opposite bearing, or tell us where such can be found? Not at all. But, instead thereof, we are flippantly told that our 'very erroneous notions' are opposed to the 'experience of the most learned observers.' Now, what is 'experience' in medicine, whether of learned or unlearned observers? Is it the mere *opinion*—the *ipse dixit* of this man or that one? Or is it legitimately the results or conclusions drawn from a careful observation and comparison of all the known facts relating to any given subject? We claim it to be the latter; and, hence, that mere *opinions* and genuine *experience* are often widely diverse from each other. For instance, Dr. Geo. B. Wood, in his recent work on *Therapeutics and Pharmacology*, says, 'It seems to be well proved, that a laborer can do a certain amount of work with less ordinary aliment, if freely supplied with beer or wine, than when water alone is allowed for drink.' This is Dr. Wood's opinion. And he tells us that *it seems to be well proved*; but he neither states the proof, nor informs us where it is to be found, and hence it can be regarded as nothing more than an expression of his belief. On the other hand, true

\* See Dr. Drake on Diseases of the Interior Valley, &c. (*Chapter on Consumption*).

† See Statistical Tables by Mr. Waring, in the March number of this *Journal*.

'experience' in relation to this point would be the result of a certain number of actual trials, in which the food, the drink, and the labor of one set of laborers had been carefully compared with those of another set. Nothing of this kind, however, is given, either by Dr. Wood, or Dr. Happoldt of the *Charleston Journal*. The nearest approximation to it which we have seen, was contained in the statistics derived from an examination of the results of laborers in some of the large brick manufacturing establishments of England a few years since. Such examination showed, that, of several hundred laborers, a part took regularly with their meals a cup or mug of *Beer*, and the remainder took nothing containing alcohol. A record had been kept, showing the amount of each man's labor daily throughout the season. On footing up this record, it was found that the number of brick actually moulded during the season by those who abstained entirely from beer and all other alcoholic liquors, averaged, per man, several thousand more than by those who took their regular rations of beer. The former had also enjoyed a greater immunity from sickness than the latter. Similar inquiries in relation to men employed in almost every department of human industry, have developed the same relative results.

Now, if alcohol is truly tonic or invigorating to the human system; if a laborer who drinks beer can do a given amount of work with less food than one who drinks only water, how are the above facts, derived from careful and extensive statistical researches, to be accounted for? Accounted for! did we say? Why, very easily indeed. Our Charleston friend does it in a single sentence, by simply alledging that we have committed the '*stereotyped error* of confounding under *one name* the various products of fermentation and distillation, and also confounding the *abuse* with the *use* of these various agents.' Inasmuch as my Address to the State Medical Society was on the Effects of *Alcohol* as a Prophylactic and Therapeutic Agent in the Prevention and Treatment of Tuberculosis, it is not easy to appreciate the bearing of the first charge in the quotation just given. Does the editor of the *Charleston Journal* mean to convey the idea that alcohol produced by fermentation is differ-



ent from alcohol derived from distillation? If so, we plead guilty to the charge of having 'confounded' them with each other; for, in our blissful ignorance, we had supposed that alcohol was properly and exclusively the product of vinous fermentation, while distillation only served to separate it from some of the accidental ingredients with which the fermentation had left it mixed. It is quite possible, however, that our critic simply meant to convey the idea that some of these accidental ingredients modified the effects of the alcohol, which is doubtless true to a very limited extent. But we ask in all candor, if it is not the *alcohol* in all the various liquors which gives them their essential qualities, and for which they are prescribed and drank?

Does Dr. Happoldt, or any other physician, prescribe or use beer, merely for the *hops* it contains; or wine, for the vegetable acids mingled with it; or gin, for its juniper; or corn whisky, for its fusil oil? If he does, we must enter a decided protest against the justice and propriety of the practice. We take it to be a settled axiom, that a physician is bound to use, as far as possible, medicinal agents of known purity and uniformity. But will any sane man pretend that we cannot get hops, and vegetable acids, and juniper, and fecula, and even fusil oil, directly from reputable druggists in a far more pure and reliable state than from the 'varying' and generally grossly adulterated products of the brewery and distillery?

We must hasten, however, to the second and last charge of our critic, viz.:—That our investigations are rendered 'erroneous' and worthless from our having 'confounded the *abuse* with the *use*' of alcoholic drinks.

This reply, if not '*stereotyped*,' has certainly been a *standing* one with the advocates of alcoholic drinks for the last half century. It matters not what view we take, or what course of investigation we pursue, the reply ever comes back the same. If the moralist points to the almost universal wreck of mind and body produced by the *use* of drinks containing alcohol: the reply is, 'You confound the *abuse* with the *use*.' If the political economist points to the lost fortunes, the squalid poverty, the social degradation, and the political corruption

produced by these agents: the same echo comes back, 'Oh, Sir, you *confound* the *abuse* with the *use* of these agents.'

Drs. Prout, Bouchardet, Magnus, Bocker, and a host of others, devise and execute varied experiments on the secretions, excretions, and temperature of the human system, while influenced by alcoholic liquors: but to the legitimate inferences drawn from all their work, the same charge comes to the ear with scarcely a variation in the quality of sound. Dr. Bocker, in his experiments, used the alcohol in doses of about one *fluid drachm*; in ours, we used wine in quantities varying from two to eight ounces, and brandy in doses of from one to four ounces. In the cases of phthisis we detailed in our Address, some had used beer in quantities varying from one to four or five glasses per day; some had used from one to three moderate drinks of distilled spirits per day for years, but never sufficient to intoxicate; while others had used all kinds and qualities indiscriminately, and to frequent intoxication: yet, in reply to all these, comes still that same ubiquitous, stereotyped, threadbare charge of 'confounding abuse with use.' With all due deference, we protest against the continuance of this course as unworthy of any man who claims to cultivate medical or any other science. If our friend at Charleston, or any one else, will present reliable statistics, or the results of well executed experiments, which fairly contradict or correct those already on record, we will most cheerfully accord to them their true value.

---

#### *To Correspondents.*

Not long since we received a letter from a subscriber at Plymouth Ind. enclosing two dollars. The writer, however, forgot to sign his name to it, and as there were two other subscribers in the same place we know not which should be credited with the money.

On the 28th of March, we received a letter through the post-office, which was accidentally lost before we learnt anything of its contents, or even the post-mark on it.

---

#### *Iodide of Zinc as a Local Application.*

In the March number of the *Peninsular Journal*, it is stated

that Dr. Pitcher, of Detroit, had recently used a solution of iodide of zinc to the conjunctiva when left thickened and granular after previous attacks of inflammation. We have also been in the habit of occasionally using the same remedy in very chronic cases, and with the most decided benefit. We apply it with a camel-hair pencil, and vary the strength in different cases, from ℥j. to ʒj. to the ounce of water. The application should not be repeated oftener than every second or third day.

Recently, we applied the same remedy (iodide of zinc ℥j. to water ʒj.) to the os and neck of the womb, and the upper part of the vagina, in two very protracted and bad cases of leucorrhœa. The application was made by means of a sponge attached to a piece of whalebone, and introduced through the speculum. It required a repetition of the application only three or four times, at intervals of three days, to suppress the discharge altogether and much improve the feelings of the patients.

---

#### *Editorial Change.*

Dr. Goadby has retired from the post of Senior Editor of the *Medical Independent*, published at Detroit, Michigan. It is announced, however, that he will continue to be a contributor to its columns.

---

### MISCELLANEOUS ITEMS.

#### *Iodine in Vomiting in Pregnancy.* By Dr. EULENBERG.

Dr. J. B. Schmitt some time since recommended the tincture of iodine in the vomiting of pregnant women, when this may be regarded as a neurosis. He gives one case in particular, in which a woman who had, in consequence of vomiting, had four abortions, and became pregnant with her fifth child. Two drops of the tincture given every two hours completely kept the vomiting under, as long as it was continued, and vomiting only returning when it was suspended. At last, a dose two or three times daily, sufficed. Dr. Eulenberg has also met with a remarkable case in a healthy woman, thirty-five years of age, who

had suffered from sickness in every pregnancy, and on this occasion had become much reduced. As she was very susceptible to the action of medicines, he ordered her tinct. iod.  $\mathfrak{3j}$ .; sp. vini r.  $\mathfrak{z}\text{ij}$ . Three drops to be taken in water every three hours. By the second day all the most distressing vomiting had disappeared, and she was again able to keep water on her stomach, and in a short time she quite recovered. The *modus operandi* of such small doses of the tincture is not very easily explained; but it is to be observed, that the iodide of potassium exerts scarcely any influence. It is probable that it only influences the vomiting, inasmuch as this is a neurosis, and abnormal condition of the nerves of the stomach.—*Medical Times and Gazette*.

---

*Dispersion of Milk.*

M. Coutenot has found the expressed oil hemp seed an admirable means for the rapid dispersion of the milk, employing it in abundant warm frictions, and then leaving the breast covered with wadding soaked in it. It is of no effect where inflammatory action, consequent upon ingorgement, has set in.—*Union Ned*.

---

*Mortality of Women in Childbirth.*

In 1848, 61 mothers died to every 10,000 children born alive in Great Britain. Since that time the mortality has progressively declined as follows: 58, 55, 53, 52, 50, down to 47 in 1854.

This is a gratifying result, and there can be no doubt that, by further care and skill, and especially by training up a class of educated nurses, the deaths in child-birth may be largely reduced from their present high number, 3,009.